

specialized habits as *Deinocerites*, is incomprehensible. Moreover a mosquito which has similarly elongated antennal segments but belonging to a distinct group, the Sabethinæ, and described in the preceding article, has recently come to light. Furthermore *Culex latisquamma* Coq. has a distinctly elongated second segment of the antennæ. As all three of these species live in crab-holes it becomes obvious that the lengthening of the antennal segments is not a "primitive" character but is correlated in some way to the mode of life of these mosquitoes. The attitude of alertness which these mosquitoes must maintain to avoid destruction by the excursions of their crustacean host may possibly account for the presence of this extra length of sensory surface.

Finally a fact bearing on Miss Mitchell's new classification of the Culicidæ by antennal characters. Unfortunately for her generalizations, in the subfamily Sabethinæ (*Trichoprosoponinæ*, Miss Mitchell) the genera *Joblotia* (*Trichoprosopon*), *Lesticocampa* and *Sabethes* have densely plumose antennæ in the male.

Class I, HEXAPODA.

Order V, LEPIDOPTERA.

IN DEFENSE OF INCISALIA HENRICI.

BY PROF. JOHN H. COOK,

ALBANY, N. Y.

In the Entomological News for April (1907) Dr. Henry Skinner has published an article entitled "*Studies of Thecla irus Godart and T. Henrici Grote and Robinson*" in which he contends that "these two names represent one variable species." Having made an elaborate investigation of these butterflies, and having published* conclusions to which Dr. Skinner has taken exception, I feel called upon to make definite and detailed reply to the article in question.

The concluding sentence reads: "From the evidence before me I am convinced that *Thecla irus* and *henrici* are one species." Let us first inquire into the "evidence" presented in support of this contention.

* Canadian Entomologist, Vol. XXXVII, No. 6 (June, 1905), p. 216.

THE EVIDENCE PRESENTED.

Point 1. — In ¶ 1 (l. 7) Dr. Skinner writes: "I have never been able to make out two species, one for each of the above names." The argument is reinforced by repetition in ¶ 3 (l. 1). "If there are two species of *Thecla* found here (Philadelphia) that might be designated as *irus* and *henrici*, I have failed to discover the fact."

Both of these statements are obviously true but can hardly be raised to the dignity of evidence.

Point 2. — In ¶ 1 (l. 18) we read: "It would seem logical to think that *henrici* was believed to be a new species because it differed from the figure of *irus* given by Boisduval and Leconte. I do not believe that the authors knew any *T. irus* in nature."

I would point out that a belief is not evidence and that this "logical" inference is based upon a debatable major premise. But the whole question should be dismissed as irrelevant and beside the point. Inasmuch as Grote and Robinson have left us the type specimen, I fail to see how a knowledge of the psychology back of the original description of *henrici* can be of any assistance in an attempt to determine the validity of the species based upon that type.

Point 3. — In ¶ 3 (l. 6) there is given a partial list of the opinions which have been expressed by various writers on the group, W. H. Edwards, Herman Strecker, S. H. Scudder, and myself.

I pass by the exclusion of the expressed opinions of J. B. Smith, H. G. Dyar, W. J. Holland, and others "who have probably not investigated" the two names (*henrici* and *irus*) and confine myself to the point at issue. From the list given it appears that the division of opinion resulted in placing Scudder and Strecker on one end of the beam and Mr. Edwards and me on the other. Thus was equilibrium maintained until Dr. Skinner threw the weight of his authority into the balance, thereby lifting Mr. Edwards and myself high in the air.

I would timidly venture the assertion that the relative value of two opinions is not to be gauged by the number and prominence of the men who hold them so much as by the number and importance of the facts upon which they are based. I may point out in this connection that, of the four eminent gentlemen in the pans, Mr. Edwards alone has bred either species; and that his more humble companion in the recent ascension has bred both species. The weights of opinions vary and it may be that the scales will respond to the specific gravity of the two unequal masses.

Point 4. — The alar expanse and the suffusion of the upper surface of the wings are stated (in ¶ 4) to afford no diagnostic characters.

With this I heartily agree; indeed pointed out the latter fact in the tenth paragraph of my former article (referred to above). What of it?

Point 5. — In ¶ 5 Dr. Skinner characterizes the stigma (which I made the basis of my definition of the species) as “a secondary sexual character that occurs in some males and not in others.” In other words, if you mix peas and beans in a bag you will find that the bag contains both peas and beans. “This not only applies to *irus-henrici* but to some other species in the genus.” We are here considering a concrete case and evidence concerning other species is inadmissible.* Let us avoid side issues. “It has no generic or specific value and is simply an individual variation.” This is an opinion and again I would remind the reader that evidence is made up of facts not of opinions.†

Point 6. — ¶ 6: On the underside of the primaries the “line running from the costa to the first median nervule . . . has no specific value and no proper correlation with other so-called specific characters given by Mr. Cook”; and *point 7*, ¶ 7: concerning the variable color areas of the secondaries beneath “there is no correlation between any of these characters . . .”

Very well then, I stand corrected. My generalizations were drawn from such specimens as I possessed and for them held good. My series at the time numbered 63 *henrici* and 106 *irus*. Every experienced lepidopterist will recognize the difficulties attending an attempt to separate two species which closely resemble each other, by reference to any single feature of the wing ornamentation without considering other features. Especially is this so when one or both of the species is variable. In the present inquiry we have an unusually

* I would find Dr. Skinner's statement of more value if he had printed the names of some of the *Thecla* which exhibit this peculiarity. I must profess the ignorance of one whose knowledge is limited and will be grateful for the facts. This is a request for information.

† I will gladly pay five dollars (\$5) to anyone who will furnish me with a butterfly of the male sex, corresponding in essential points with Grote and Robinson's type *henrici*, which has a stigma. I will pay an equal amount for any male butterfly of the genus *Incisalia* (Minot) taken in the eastern United States or Canada, without the stigma and which does *not* agree in essentials with the above named type. This is not a reward; the amount offered represents simply the value to me for study of such specimens, if they exist.

constant species (*henrici*) and a very variable species (*irus*). In my article (Can. Ent., June, 1905) I sought to indicate the principal characters which I had *always* found associated in *henrici*. I have not since found it necessary to modify my characterization of that species. My knowledge is confined to the 188 *henrici* in my own collection, the specimens in the New York State Museum, the Museum of Natural History (New York City), the National Museum, eleven private collections to which I have access, and a few individuals which I have sent to other collections. Still I am not wedded to the generalizations and am perfectly willing to abandon each and all of them upon the presentation of proper evidence; however — not otherwise.

Concerning the differentiating characters which I gave for *irus* I frankly admit that each may prove unreliable when considered alone* but I have yet to find a specimen which does not exhibit *some* of the differentiating characters. My examination has been confined to the material in the collections before mentioned, the J. A. Lintner Memorial Collection and 634 specimens in my own cases. It is by no means impossible that Dr. Skinner, with a larger or more complete series has been enabled to reach conclusions more valuable than those which I have published.

To sum up: points 1, 2 and 3 may be dismissed as irrelevant; point 4 is conceded, it has no weight as an argument; point 5 does not fall under the head of "evidence" — it is an opinion; points 6 and 7 are open to question. I would point out that Dr. Skinner's failure to find correlated differences among the butterflies does not prove that such differences do not exist; and *even if they did not exist, the main thesis* (that *henrici* is identical with *irus*) *would still be unproven.*

THE EVIDENCE NOT PRESENTED.

It may be pertinent also to examine the evidence which Dr. Skinner has withheld. In reply to the seven points of evidence which he has adduced in support of his contention, I would present seven other points as follows:

1. *Henrici* eggs differ constantly from *irus* eggs.
2. *Henrici* larvæ in the second stage differ constantly from *irus* larvæ in the second stage.

* For instance the criterion of the projection of the basal-area (secondaries beneath) between the median nervules, has failed in four cases out of more than eight hundred — less than one half of one per cent.

3. *Henrici* larvæ in the third stage differ constantly from *irus* larvæ in the third stage.

4. *Henrici* larvæ in the final stage differ constantly from *irus* larvæ in the final stage.

5. *Henrici* chrysalids differ constantly from *irus* chrysalids.

6. *Henrici* "breeds true"; *henrici* ♂ mates with *henrici* ♀ and the progeny are *henrici*.

7. *Irus* "breeds true"; *irus* ♂ mates with *irus* ♀ and the progeny are *irus*.

Of these seven facts the first five were (or should have been) known to Dr. Skinner before the publication of his views in the Entomological News for April, for I communicated them to him early in February. The letter was enclosed in an envelope with my address printed in the upper left hand corner and has never been returned to me by the postal authorities. Even under the charitable assumption that this letter never reached its destination, Dr. Skinner can hardly plead that the facts were unknown to him without laying himself open to the charge of culpable ignorance of the literature of his subject, for W. H. Edwards published the life-history of *henrici* more than twenty-five years ago* and the life-history of *irus* appeared in the Canadian Entomologist in 1906.†

Three hypotheses may be entertained in an endeavor to account for Dr. Skinner's attitude: (A) he has ignored the above facts — in which case his argument is unscientific, for it is surely unscientific to suppress evidence that does not square with a preconceived notion; (B) he doubts the facts — which is discourteous as well as unscientific; or (C) he has chosen to interpret them otherwise than as establishing the specific validity of Grote and Robinson's *henrici* — which is merely absurd.

NOTE BY THE EDITOR. (= H. G. Dyar)

The distinctness of these two *Thecla* is proved on the adult characters adduced by Professor Cook. The stigma of the male being present in one and not in the other. This is, as Dr. Skinner says, a secondary sexual character; but it is of absolute specific value. It has generally been used as of generic value, and the reason we agree with Dr. Skinner that it should not be so used, is not because it is variable or inconstant, which is not the case, but because as a matter

* Papilio, I, 150-152, Oct., 1881.

† Vol. XXXVIII, No. 5 (May), p. 141 and No. 6 (June), p. 181.

of policy and choice we prefer to found genera on characters exhibited by both sexes. Many will not take this view. In fact most of the families of North American Lepidoptera have been heretofore classified on secondary sexual characters. In this view, our two *Thecla* are not only specifically, but generically distinct. Of course, the larval differences are additional proof, if any were needed. We infer that Dr. Skinner, not having studied the larvæ, has supposed that their characters were negligible.

SOME NEWFOUNDLAND GEOMETRIDÆ, WITH DESCRIPTION OF A NEW VARIETY.

BY LOUIS W. SWETT,
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In collecting a series of the common *Sciagraphia granitata* Gn. I was struck with the great diversity of coloration and markings, the colors varying from violet gray, yellowish, dark olive, light gray to pure silvery white and some with brick red markings. In a lot from Newfoundland, through the kindness of Mr. Owen Bryant, I found three of a very distinct form differing from any of the numerous descriptions and from others from the same locality. At first I feared to describe them, as there are so many synonyms in this group, but after reading through all the descriptions I find certain features which make these very distinct. The only description that at all approaches this variety is *submarmorata* Walk. (Cat. Brit. Mus., p. 887, vol. 23, 1861), but the markings and color of lines render it distinct. Below I append my description.

Sciagraphia granitata, var. *oweni*, new.

Expands 1.2 inches. Color above silvery white with very large black dots and wide smoky black lines. On costa, four black patches more lengthened than usual, white between. Basal line of fore wings smoky black (width of the body) showing in three prominent spots. Mesial band smoky, straight at costa, wider than on inner margin, including black distal dot, the black dots show plainly on veins. Beyond discal dot a narrow white sinuate line bordering the very broad smoky band which runs almost straight from black apical patch to inner margin, widening after spot between veins 3 and 4 as it approaches. Beyond this the border is marbled with black and white striations (more marked than inside mesial band) sometimes running into cloudings. The veins at base of fringe marked with small dots. Hind wings heavily